Interactive comment **on "Darwinian hydrology: can the methodology Charles Darwin pioneered help hydrologic science?"** ***by*** **C. Harman and P. A. Troch**

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This paper (Harman and Troch, 2013) is a well-structured and generally clear presentation that is also well referenced in regard to what I take to be the current paradigm for hydrological science. Because the paper is largely philosophical in nature, the usual criteria for the review original scientific research results are not applicable, so instead emphasis will be placed on philosophical commentary. Philosophy can be defined as the raising of questions about concepts that are generally presumed without question when going about some activity (Nagel, 1987), which in this case is the activity of scientifically pursuing hydrological understanding, particularly for watersheds. Thus, my review comments will be in the spirit of philosophical questioning, as productively invited by this paper. My comments will necessarily extend beyond the specific points raised in the paper to question presumptions underlying those points, presenting contrary positions that, in some cases, are well established in the contemporary philosophy of science literature, but, in other cases, are matters of ongoing philosophical debate. Some of this material may be unfamiliar to practicing scientists, many of whom seem to ascribe unquestioningly to philosophical doctrines that were popular in the mid-twentieth century but have since been largely discredited by contemporary philosophical work. For those interested Godfrey-Smith (2003) provides an authoritative and useful review of the latter.

The paper under review takes on the commendable goal clearing up confusion associated with what a number of recent papers have termed a "Darwinian" approach to hydrological science. This is very useful because many of these same papers draw attention to the need for hydrological science to place more emphasis on understanding the complexity of the hydrological cycle and its component processes by focusing on the specifics of individual watersheds, as opposed to combining accepted physical principles with what are presumed to be general characteristics of watersheds in order to satisfy a goal of making operationally useful predictions for engineering purposes. It is very clear that hydrology can be greatly advanced as a science by including more of the methodologies of productive hypothesizing that are exemplified in the approaches that were used by Darwin that eventually led him to formulate his theory of evolution by natural selection. Thus, I rate this paper as excellent for drawing appropriate attention to these methods as a complement to (but certainly not a complete replacement for) the very pronounced physics-mathematical overemphasis that characterizes so much of modern hydrology.

It is also excellent that the current paper emphasizes the methods used by Darwin in actually doing his science, though, contrary to the title, it was generally was not Darwin himself who "pioneered" these methods. I think it is particularly important that the paper directs attention to a scientific goal for hydrology that would place much more emphasis on the formulation of bold hypotheses to explain causally the patterns that are discerned in the course of hydrological investigations of populations of individual watersheds as they vary across time and space. Unfortunately I also think the paper uncritically repeats from other scientific literature many philosophically confused ideas about how these hypotheses are formulated and used in the "historical" sciences that were central to the work of Darwin. Moreover, the paper's prescription for expanding hydrological understanding across the time dimension largely ignores potentially very important contributions from a well-established branch of the hydrological sciences that is intrinsically Darwinian: paleohydrology.

Many years have now passed since the late Vit Klemeš raised philosophical issues critical to achieving a scientific hydrology (e.g., Klemeš, 1986, 1997, 2000). Though Vit Klemeš himself was praised for being "the conscience for hydrology," it can be argued that there has been relatively little change in the attitudes and practices that he criticized. Can what aspires to be scientific hydrology, by adding some Darwinian methodology, be absolved from the charge of scientific "dilettantism" that Klemeš (1986) so effectively laid upon it?

"Newtonian and Darwinian" science

Without doubt it is likely that, if asked to name the two researchers whose work was of greatest importance for the development of modern science, most scientists would immediately name Newton and Darwin. That these two scientists' methods and theoretical worldviews could not have been more different in philosophical approaches to science has proven to be an intriguing puzzle for both philosophers of science as well as those scientists who occasionally think about the philosophical underpinnings of their work. To the confusion of the latter, many of the philosophers of the last century focused their attention exclusively on the methods of Newton and his many successor physicists, some of whom continue to proudly proclaim, in numerous professional and popular writings, the merits of the methodologies that Newton presumably exemplified as the very best or even the only valid methods wherein to do science. Mathematics, the science that draws absolutely necessary conclusions, is central to these methodologies, but pure mathematics can and sometimes does derive its necessary conclusions from completely arbitrary and unnatural premises. Thus, it falls to physics, the most mathematical of all the natural sciences, to act as the scientific exemplar for those schools of philosophy that restrict the definition of knowledge to universal, necessary, justified, and true belief. This is understandable for those philosophers for whom the holy grail has long been the achieving of absolutely certain knowledge, whether that be in the Platonic world of the forms or in that which might be deduced from the Cartesian *cogito ergo sum*. However, this attitude is incompatible with much of scientific practice, as was classically argued by the American pragmatist school of philosophers, including Peirce, James, Dewey and others (see, e.g., Dewey, 1930).

One has to take great care to avoid reading too much into the writings of philosophers who most commonly read about science (mostly what is written by physicists), but who rarely actually engage in the spirit of doing it. Science is not and cannot be about absolute the kind of true and certain knowledge of the philosophers for the simple reason that the activity of science, and science is an activity not a body of facts, is a search for truth, and not the actually achieving of it. If one could have the certainty of absolute truth, then there would be nothing more to search for, in which case there would no longer be science (e.g., Horgan, 1996). This point should not be confused with the fact that scientists pragmatically hold that many of their fundamental principles are highly reliable (Ziman, 1978). After many wrong turns, most modern philosophers of science have relatively recently come to realize something that had long been intrinsic to the methodologies, and presumably the working successes, of the most productive scientists: that all knowledge is fallible. Much more can be said about this, but this recognition of fallibilism as fundamental to science is important for issues relating to presumptions underlying the goal of predictive modeling, views on confirmation and falsification, and other important philosophical concerns that come to play a role in scientific practice.

While it is commendable that the paper under review seeks to clarify what is meant by "Newtonian" and "Darwinian" hydrology, and particularly to correct ways in which the designation "Darwinian hydrology" could be a source of confusion and misinterpretation, the metaphors chosen for this agenda may well introduce more problems than they resolve. Moreover, the paper applies the designations "Newtonian" and "Darwinian" to multiple, diverse concepts, including the method for doing science (e.g., p. 6408, line 9; p. 6412, line 1, page 6420, line 9; p. 6429, line 7), an "approach" to science (page 6408, lines 2-3, 5, 7, 11; page 6410, line 9; page 6420, lines 8-12; page 6433, lines 16-18), worldviews (page 6409, line 17; page 6410, lines 5,10, 17), and paradigms (page 6414, line 8). Is the paper about the methods that Newton and Darwin used to discover their theories? Is it about the worldviews embodied by those theories? Is it about the kinds of explanation that are provided by those theories? Is it about all of the above?

The "Newtonian" worldview and hydrology

The aim of the "Newtonian worldview" for hydrology is stated in the paper to be to "…develop physically-based models of hydrological behavior with a level of precision approaching that of Newton's mechanics or derived from Newton's 'first principles'… " (page 6410, lines 10-12). This seems to be the currently prevailing hydrological science paradigm that originally had much more of an engineering motivation than a scientific one. The problem with calling this "Newtonian" is that Newton was a scientist, not an engineer. He was interested mainly in discovering the laws of mechanics, not in actually applying them to making practical predictions (which need to be as "precise" as can be economically achieved for their engineering application). Engineers are not interested in learning something new about nature by the use of predictive tests; they want to apply what is already known to make predictions that will match reality as closely as possible. In contrast, because Newton was a scientist devoted to discovering the realities of nature, he was not particularly interested in applying his own principles to achieve the most accurate possible predictions. It is only possible to learn something new about reality from a predictive test, i.e., to use the prediction scientifically, if the predicting and the testing are part of a process that begins with a seemingly workable hypothesis. If the hypothesis fails the test, then one at least has learned something new, specifically that something is wrong either with the hypothesis or with various auxiliary presumptions, though the problem of underdetermination (see discussion below) may logically exclude the possibility of knowing exactly what is wrong. (Note, however, that what is described here should not be confused with the falsificationism advocated by Sir Karl Popper—see further discussion on this point below.)

Newton was able to achieve a kind of confidence in his scientific reasoning processes because, as is the case for much of physics and chemistry, he was able to do the necessary testing under very carefully controlled conditions in a laboratory setting. The necessary controls for such testing are not available in the historical and interpretive sciences that characterize the work of Darwin as well as much of the Earth sciences, particularly geology. This leads to very different emphases in methodologies, though this does not mean that the interpretive historical methodologies of the Earth science are any less relevant to achieving scientific understanding than are the theoretical-experimental approaches of physics (Cleland, 2002, 2011, in press). Unfortunately, the paper under review does not mention much of the modern scholarship on the nature of the historical sciences, and the one paper that is cited (Kleinhans et al., 2010) advocates some philosophical positions that are in certain respects at one pole of what is an on-going debate in philosophy of science.

Applying Newton's example to hydrology, one can see that a goal of achieving absolute certainty in predictive success, or the related goal of reducing uncertainty, is an engineering holdover for a hydrological enterprise that currently aspires to be a science. Combining the engineering goal of reduced uncertainty with the scientific goal of discovering something new about nature produces a hybrid that is neither good science nor good engineering, but is instead a combination that is but an impoverished cousin to each. In conclusion, what is termed a "Newtonian worldview" in the paper under review (page 6410, lines 10-12) is not what is exemplified in the actual methods and approaches used by Newton himself. Instead, it is really a kind of applied physics, modified to achieve the minimization of uncertainties needed for application, but incapable on its own of achieving the kind of major advances in understanding that characterize the effective science done by both Newton and Darwin, while at the same time also lacking in the rigorous attention to tolerances, limitations, and factors of safety that characterize responsible engineering.

The Darwinian worldview and hydrology

It should be pointed out that it has now been about 20 years since a paper written by the eminent hydrologist Luna B. Leopold proposed consideration of the Darwinian worldview in a hydrological context (Leopold, 1994). It is a telling commentary that the hydrological scientific community, within the blinders imposed by its Newtonian paradigm (as this is defined on page 6410, lines 10-12), seems to have largely ignored this paper by one the last century's most important hydrologists. Drawing upon Mayr's (1991) summary of the main ideas in Darwin's theory, Leopold (1994) proposed that rivers (and for the case at hand we can easily substitute "watersheds"), rather like living organisms or species: (1) do not consist of large classes of identical objects, like the atoms that are analyzed by physicists and chemists, but instead as individuals who are not identical with one another; (2) should not be considered as a type, but rather as a variable population for which the variability changes by a shift in mean values; (3) are individuals that each are products of a history that reflects unique conditions from its past; (4) exist in populations that react to changing environmental conditions, and (5) involve processes that are teleomatic (end-directed) or teleonomic (goal-directed).

The paper under review reiterates many of the same Darwinian themes that were earlier identified by Leopold (1994), including points about essentialism and the origins of variability within populations (pages 6412-6415) as well as Darwin's emphasis on "the documentation and explanation of the spatial variations of individuals" (page 6412, line 23). However, the paper does not mention the telomatic or telonomic themes (following Mayr's [1991)] terminology) that Leopold (1994) emphasizes. Some discussion of Leopold's seminal contribution to "Darwinian hydrology" would seem to be warranted.

The paper under review rightly draws attention to the limitations Newtonian hydrological science, though not in the way that I have stated above. As a partial antidote to the current Newtonian overemphasis in hydrology, the paper specifically advocates a "Darwinian approach" that will seek "to understand a variety of complex systems in terms of their historical processes of change" (page 6408, lines 9-10) by following Darwin's lead in "focusing attention on the patterns of variation in populations, seeking hypotheses to explain these patterns in terms of the mechanisms and conditions that determine their historical development, using deduction and modeling to derive consequent hypotheses that follow from a proposed explanation, and critically testing these hypotheses against new observations" (page 6408, lines 11-15). Unfortunately, this statement, while correct in its focus on variation in populations and the importance of testing (broadly construed), is erroneous claiming that deduction and modeling can derive consequent hypotheses. A hypothesis is something that brings a new, tentative explanation to a scientific problem. It is a matter of formal logic that a deduction can do no such thing. Deduction is a kind of truth-preserving logic: it preserves whatever truth was present in the original premises. It does not create a new pathway to truth, i.e., a hypothesis. A new hypothesis can only be generated by the mode of reasoning that is known as "abduction", a formal term that is mentioned only once in this paper (page 6418, line 21), and only then in a context that comes from Kleinhans et al. (2010), who make the mistake of claiming that abduction is a part of what philosophers call "inference to the best explanation." A very brief introduction to the actual nature of abductive inference can be found in De Waal (2013, p. 63-66).

Darwin's methods

The paper under review argues, paralleling suggestions by Gould (1983), that the following attributes characterize Darwin's method of doing **historical science**: "extrapolating mechanisms, space for time substitution, and looking for patterns in history" (page 6408, lines 24-25). However, these ideas are not unique to Darwin, being characteristic of geological reasoning in general, as reviewed by Schumm (1991). Nor was Darwin the "pioneer" in developing these ideas and indeed many other aspects of the scientific methodology that he employed. There has been rather extensive scholarship on this topic (e.g., Ruse, 1975; Thagard, 1977), not all of it understood by and/or explained in the sources that are referenced in the paper under review.

Unfortunately the paper under review ascribes to Darwin methodologies that he really did not espouse. One of these is falsificationism (page 6408, lines 19 and 28; page 6410, line 7; page 6418, line 21; page 6434, line 12). Another is hypothetico-deductivism (page 6420, lines 15-16). These problematic assertions get mixed in with otherwise excellent discussions of methods that are indeed exemplified in Darwin's work. For example, on page 6420, lines 12-15, in another of its several statements of the Darwinian method, approach, worldview, etc. the paper claims that the aim for the Darwinian approach is "…to provide an explanation—derived from the historical co-evolution of the landscape and the legacies of the past over many timescales (from geological to human)—for the patterns of variation in hydrologic behavior within a population of watersheds". However, the statement of this worthwhile aim is followed (page 6420, lines 15-16) by philosophically problematic references to the "hypothetico-deductive method that Darwin advocated" and "just-so stories" (see below for further discussion).

Falsificationism

Falsificationism, a doctrine advocated by the philosopher of science Sir Karl Popper (e.g., Popper, 1959, 1963), has become rather popular with many scientists, so much so that the criterion of "falsification" has become a kind of mantra, presumed without question, and even applied to critiques of scientific papers and research grant proposals. This popularity contrasts sharply with the almost total rejection by modern philosophers of science of Popper's arguments for the logical use of falsification in science. A discussion of the many relevant issues can found in Godfrey-Smith (2003, p. 63-70).

One of the issues relevant to falsificationism is of general importance for all notions of confirmation and falsification by correspondence tests, particularly the predictions that are made by theoretical models in a Newtonian mode. This issue is known in philosophy as the Duhem-Quine Thesis or as “the underdetermination of theory by evidence.” Unlike the underdetermination issues that are discussed by Kleinhans et al. (2010), the Duhem-Quine Thesis is a strong form of underdetermination that cannot be resolved by more and better data, better models, more “predictive understanding”, etc. This Duhem-Quine Thesis is the reason why it is impossible to “verify” model predictions as opposed to “validating” the predictions made by models (Oreskes et al., 1994).

As pointed out more than 50 years ago a book by P. Duhem (1954) and also in a much celebrated philosophical paper by the logician W. V. O. Quine (1951), it is not possible to isolate individual hypotheses and theories when making real world tests. Whether done in the field or in a laboratory, any concrete test of a theory, model, or hypothesis involves numerous auxiliary assumptions about boundary conditions, the nature of instruments, potential sources of interference, etc. Moreover, many of these auxiliary assumptions will be unstated, highly theoretical, poorly understood, or even completely unknown. Because it will never be clear from a failed prediction whether it was the primary hypothesis (or model) that produced the negative result or whether it was one or more of the myriad of auxiliary hypotheses, it will always be the case that the test cannot be conclusive in regard to advancing or rejecting any particular hypothesis or presumption.

The Duhem-Quine Thesis would appear to be rather devastating for a scientific program that would rely upon the testing of hypotheses through their predictive success or failure, but this formal logic thesis actually has limited application to science in general. Duhem (1954) argued that the strong underdetermination problem only arises in physics and parts of chemistry, since it is restricted to those forms of testing that do indeed require the very strict isolation of hypotheses in regard to very specific model predictions. There are many other kinds of testing done in science besides the logic-limited prediction-to-confirmation/falsification type (see, e.g., Baker, 2012), and most of the testing done in the historical sciences, including much of the science done by Darwin, is not of the type that is subject to the logical limitations imposed by the Duhem-Quine Thesis. That the historical and interpretive sciences of the Darwinian worldview do not suffer from the logical underdetermination limitations that apply to sciences of the Newtonian worldview supports the thesis of the paper under review: that hydrology, to be most effective as a science, needs to balance its overemphasis on the Newtonian approach with more attention to investigative research done from a Darwinian perspective.

Hypothetico-Deductivism

As noted in the interactive review comment by Westhoff (2013), there is some vagueness the paper under review as to whether the Darwinian method is merely the same as the hypothetico-deductive model that so often gets presented in textbooks as the one presumed method for all the sciences. It appears that the claimed association hypothetico-deductivism with Darwin comes from earlier historical scholarship by Ghiselin (1969), which also underpins the more recent review by Ayala (2009). There are two problems with this claim. The first involves the program of historical scholarship that attributes to Darwin the kind of hypothetico-deductivism that is associated with philosophical concepts of the logical empiricist tradition that became the predominant view in philosophy of science during the mid-twentieth century, which was the period in which Ghiselin did his historical research. That research can be thought of as an example of what is termed "whiggish history," in which the historian interprets past historical phenomena, not so much in the context of their own time period, but rather as a kind of progress toward what is presumed to be the best qualities of the historian's own times. This results in a controversial mode of historiography that is the subject of considerable criticism by modern historical scholars.

Though a digression into Darwin historical scholarship would be interesting at this point, the more relevant problem related to the paper under review is that the hypothetico-deductive (HD) method of science is now known to be a flawed representation of how productive scientific inquiry actually works (see discussion by Godfrey-Smith, 2003, p. 69-70). While the deductive part of the HD method is generally non-controversial, the roles of both abduction and induction, and especially the understanding about how each of these modes of inference work in scientific practice, have been sources of continuing philosophical confusion and debate, extending back to the time of Darwin and before. Ironically, in the late nineteenth century Charles Sanders Peirce, the great American logician and practicing geophysicist, had already recognized the logical flaws in what later became hypothetico-deductivism, a view of scientific methodology closely related to the philosophically discredited logical positivist/empiricist program of the early and mid twentieth century. As envisioned by Peirce, and recently summarized by De Waal (2013, p. 66), a short statement of scientific reasoning is the following: "…abduction furnishes us with explanatory hypotheses, or theories, deduction draws out their logical implications, and induction verifies (or falsifies) these implications, and by doing so verifies (or falsifies) the hypothesis." While further elaboration on Peirce's view of induction would involve discussion greatly exceeding the limitations for this commentary, more attention needs to be given to the key role of abduction, which involves the formulation of explanatory hypotheses, since that is the key element by which any scientist, including both Newton and Darwin, is able to achieve progress in the quest for understanding the nature of things.

Hypotheses and the interpretive-historical sciences

There are probably many who would argue that the "holy grail" for hydrology is to discover hydrological laws (e.g., Dooge, 1986). In this regard, it is telling that when someone once asked the late Richard Feynman how he would go about discovering a new law of physics, the famous Nobel-prize winner did not reply by giving a description of the HD method. Feynman's answer was that first he would guess what the law is, then he would deduce consequences from it, and finally he would test those deductions. Obviously the key component in Feynman's statement of the scientific method is what he terms, in his famously quirky style, a "guess." As noted above, what Feynman terms a "guess" fits exactly into the flow of scientific reasoning in the role that Peirce ascribes to abduction. Abduction, the reasoning process that formulates a scientific hypothesis (the highly informed scientific "guess" as to the causative system that can explain some observed phenomenon), is the most critical part of the scientific process, and this is much more the case for the Darwinian styles of doing science than the Newtonian. Peirce himself even classified the sciences in terms of these modes of inference. He classed physics as a deductive science, and the more descriptive biology of the late nineteenth century was classed as inductive. It was the historical/interpretive science of geology that Peirce classed as abductive.

The philosopher Carol Cleland (2002, 2011) argues that historical sciences like geology ("Darwinian" in the context of the paper under review), in contrast to the theoretical/experimental sciences (i.e., "Newtonian"), use a methodology of evidential reasoning that employs (1) multiple hypotheses to explain (generally in terms of their causes) the effects of past events that are discovered in the field, and (2) a search for a "smoking gun" that will discriminate among these hypotheses. Unlike the traditional "scientific method" the evidentiary process of the historical sciences is less concerned with prediction followed by confirmation/refutation than it is with the explanatory power associated with various hypotheses. These points are highly relevant to the concept of Darwinian hydrology because Darwin was very much a geologist (Herbert, 2005) as well as a biologist. The paper under review does present a limited account of Darwin's geological background, specifically his work on coral reefs (Darwin, 1842), but this only begins to introduce the very profound effect that geological thinking had on Darwin's way of doing science. (It is an interesting sidelight to note that Newton's approach to the geological problem of the age of the earth was to quantify the life spans of individuals mentioned in the bible.)

Because the historical sciences are less concerned with "what we can say about nature" and more with "what nature says to us" (Baker, 2000) they have less to do with the logic issues of how we make our statements, i.e., formulate our predictive models, etc., than they have to do with how we interpret the realities that nature presents to us in the course of our investigative inquiries. As already noted, this means that the historical sciences will be engaged in modes of testing that is are not subject to the logical limitations of the Duhem-Quine Thesis. Cleland (2011, in press) claims that the dominant form of explanation in the historical sciences is by common cause in which otherwise puzzling assemblages of what she terms the "traces" of past phenomena (such as the fossils studied by paleontologists) get attributed to a common cause. Of course, this is a form of abductive inference, and such unification of diverse phenomena is recognized in the paper under review as an attribute of the Darwinian approach. However, Cleland takes this point much further by making the claim that the common cause mode of explanation used in the historical sciences, unlike the logical claims in theoretical/experimental sciences ("what we say to nature"), is based in the actual causal structure of the world ("what nature says to us") as well as the nature of time. These bases produce what philosophers term "the asymmetry of overdetermination": that local events can evidentially *over*determine their past causes, but that they *under*determine their future effects. To illustrate this Cleland (in press) uses the example of a volcanic eruption that produces an immense array of evidence, geological "traces", relating to the details of the eruption's past occurrence (i.e., the understanding is *over*determined). In contrast, the theoretical prediction of some particular effect of a future volcanic eruption is highly *under*determined in that there are many possible ways in which causative factors can combine to generate that effect.

Cleland's arguments are the subject of current debate among philosophers (e.g., Turner, 2004, in press), but they raise very important issues that show how historical sciences of the Darwinian type operate very differently from the Newtonian theoretical/experimental sciences. More importantly Cleland's arguments show that these science have some advantages over the latter, particularly in regard to the access that they provide their to "what nature is saying to us" as opposed to "what we are saying about nature."

There are many other issues of importance that are related to historical sciences like geology. These include the role of hypotheses (Gilbert 1886, 1896; Chamberlin 1890; Baker, 1996a), the nature of interpretation (Frodeman, 1995), various roles of abductive reasoning (Von Engelhardt and Zimmermann, 1988), and the practical logic of using signs (what Cleland terms "traces") as indicators of past processes (Baker, 1999). Full discussion of these issues is beyond the scope of these review comments, but they cannot be ignored when considering a transition from total reliance on the Newtonian approach in hydrology and inclusion of Darwinian methodologies.

Overemphasis on prediction

Darwin did not have a program to use his theory of evolution to predict the future course of evolution. He would not be particularly interested, for example, in a program to predict whether, with some reduced level of uncertainty, we humans will eventually evolve into cyborgs. Thus, I find the concept of having "a Darwinian approach for prediction in ungauged basins" (page 6409, line 27; page 6410, line 1) to be a mixing of incompatible elements. This emphasis on predictions, as opposed to the actual type of historical science done by Darwin, may arise from a need to be compatible with ideas that underpin the recently completed IAHS program for Predictions in Ungauged Basins (PUB) that seeks "to predict and understand watershed behavior in areas without long gauging records (Sivapalan, 2003) and under changing conditions" (page 6409, lines 21-23). The PUB program seems to me to be almost exclusively dominated by the Newtonian viewpoint, given its emphasis on uncertainties and the predictive power of models, even though that program mixes in some scientific goals for improved understanding and realism (Hrachowitz et al., 2013) that I suggested earlier in this commentary merely contribute to creating a kind of impoverished science/engineering hybrid.

The time factor, change, and the role of paleohydrology

The paper under review concludes (page 6433, lines 22-23) that Darwin's "crucial step" was not to limit his study to the physiology of individual species, but to also study "the variations in physiology though space and **time**" (emphasis added). Though mention is made (page 6434, line 6) of the limitations of documenting watershed behaviors "ahistorically, as brute facts" (presumably as is done by the Newtonian approach), this paper makes no mention of an entire branch of hydrology that specifically deals with the time dimension by making real-world discoveries in regard to the reality of hydrological processes in their individual circumstances. That unmentioned branch of hydrology can play a direct role in contributing understanding to exactly what the paper under review claims to be the "quintessential problem for hydrology…to answer: how to use the observable structure and function of watersheds to infer the pathways of their history" (page 6411, lines 17-18). The language for this statement is partly borrowed from an essay by Gould (1983) that discusses Darwin's methodology, but its translation to hydrology leaves as vague one critical portion of the Gould quote. In his essay Gould did not limit the quintessential problem for evolutionary theory to just the use currently observable anatomy, physiology, behavior, variation and geographic distribution of modern organisms to infer the pathways of history. Gould also included in that list the "fossil remains in our geological record" (page 6411, line 14), and such fossil remains, of course, derive from currently unobservable real organisms. Experienced scientific investigators of this evidence, i.e., paleontologists, studying it in the context of reconstructed geological environments, can discover the relevant patterns of structure, distributions, processes, etc. that were associated with the various populations of the real organisms that produced them. A hydrological science that ignores for watersheds what are analogous to the "fossil remains" studied by paleontologists can hardly be called "Darwinian."

I propose then that there is a "missing link" in what obviously needs to be added to define the "quintessential" problem of hydrological theory, and that this link is paleohydrology, which is the branch of hydrology that studies the past evidence of hydrological changes in much the same way that paleontology functions to study the past realities of evolution, a process of change that operates through time. Not only is mention of paleohydrology missing in the paper under review but this branch of hydrology also has been largely ignored or marginalized in much of the conventional mainstream of the hydrological sciences, probably as a result the prevailing Newtonian emphasis on ahistorical brute facts. For example, the recently completed 10-year IAHS program on “Prediction in Ungaged Basisn (PUB)” (Sivapalan, 2003) was based on the premise of data scarcity, though what was considered to be hydrological data was arbitrarily limited to consist of active measurements of current hydrological variables according to a Newtonian worldview. It never seems to have been seriously contemplated that additional data of relevance to a broadly construed hydrological science might be obtained from natural archives using the science of paleohydrology. Instead, this presumption of data scarcity was used to justify an extensive program with a non-Darwinian focus on modeling, predictive power, and reduced uncertainties (Hrachowitz et. al., 2013). While it was not wrong to have the latter as a portion of a scientific program (the “what we can say about nature” part), it definitely was unscientific to arbitrarily exclude from that program a potentially important source of real-world data (the “what nature can say to us” part).

For Darwin’s evolutionary biology scientific understanding is enriched by access to the natural archive of fossil evidence that is studied by paleontologists. There is no question that this archive provided key insights to Darwin for making the discoveries that led to his revolutionary transformation of the biological sciences. Can the hydrological sciences, which presumably aspire to a similar level of fundamental scientific discovery as that achieved by Darwin, continue to largely ignore its own equivalent to paleontology: the science of paleohydrology, with its long tradition of using Darwinian approach to science? The paper under review does suggest that “regimes and filtering” (page 6421-6423) can provide something of a hydrology analog to Darwinian methodology for dealing with variations across time, but this suggestion is far from an embracing of paleohydrology in the same way that Darwin’s evolutionary paradigm embraced paleontology.

Though it is possible to do a kind of science, here termed “Newtonian,” that makes assumptions about the real world and then extrapolates through unreal future time with the aid of quantitative, predictive models, even that kind of science needs to make use of any and all available real-world data, if only relate that data to its modeling goals. Were it to ignore the only major archive of data on change through time that actually signifies past changes over significant time periods any future scientific program focused on change could be even more anti-Darwinian than was the PUB program. In this regard it is telling that change in hydrological environments is to be an important emphasis for a new IAHS program for the scientific decade 2013-2022 “Panta Rhei—Everything Flows”: Change in Hydrology and Society (Montanari et al., 2013). However, like its predecessor, the PUB program, the “Panta Rhei” initiative makes no mention of paleohydrology, unless it is a single oblique reference to “soft and proxy data” for modeling purposes (Montanari et al., 2013, page 7). If this designation is to suggest that paleohydrological data are “soft”, then that pejorative that needs to be defended in scientific terms. If it is to suggest that such data only serve as “proxies”, that designation needs to be justified logically relative to the great variety and complexity of paleohydrological data that are abundantly referenced in the scientific literature. In any case, the lack of substantive paleohydrological input makes the program anti-Darwinian.

A complete review of paleohydrology would be way beyond the scope of these review comments, but it can be noted that the field received its formal name about 60 years, also as a result of work by Luna B. Leopold (Leopold and Miller, 1954). Paleohydrology is the science concerned with past occurrences, distributions, and movements of continental waters. It is the science that links conventional hydrology to the sciences of Earth history and past environments (Schumm, 1967). The linking extends in both directions, in that modern hydrological data and procedures can be useful for creating models of past water processes, while patterns of past water processes should be useful for extending understanding of current water processes, especially as that understanding is extrapolated over broad timescales into the future (Baker, 1996b). Because of recent technological advances paleohydrology has been experiencing revolutionary changes its ability to establish quantitatively both the timing and process details of past hydrological phenomena. Examples include advances in and the global expansion of paleoflood hydrology (Baker, 2006, 2008, 2013; Benito and Thorndycraft, 2004; House et al., 2002).

"Just-So" Stories

While this may seem to be a minor point, I am unclear as to why this terminology is being used (page 6408, line 17 and page 6420, line 16), unless perhaps because some Newtonian hydrologist might be inclined to apply it to a Darwinian explanation. Nevertheless, there are two reasons why the use of this term may be inappropriate. The first reason suggests that any use of the term will require extensive clarification, and the second reason suggests that use of the term is probably best eliminated altogether.

The first reason is that the term "just-so story" is a pejorative that gets applied generally to narrative forms of explanation that are sometimes used in historical sciences like paleontology and geology. However, narrative explanations are potentially unscientific only when they are not subject to testing. Merely because physicists do not use the narrative form of explanation does not make such explanations intrinsically unscientific. The attitude associated with the latter claim is reminiscent of that once conveyed by Ernst Rutherford who stated to the effect that all science is either physics or it is mere "stamp collecting." Of course, the reply to this combination arrogance and ignorance is that the statement is almost certainly true in one regard: if Rutherford himself were to try to do paleontology or some other historical science, then the result probably really would be intellectually equivalent to stamp collection. Referring to "just-so stories" brings discourse to a similar level.

The second reason that discussion of "just-so stories" may be inappropriate relates to specific applications of this term that are made to Darwin's work. Just as they make wide use of the falsificationism of Sir Karl Popper to claim that Darwin's evolutionary theory is unfalsifiable and therefore not scientific, something that Popper himself once claimed (and later regretted), anti-Darwin creationists also misleadingly refer to many of Darwin's justifying arguments for evolution as "just-so stories". They base the latter claims on misuse of the term by some of the philosophically naïve scientists, mainly of the "Newtonian" persuasion, responsible for viewpoints that were noted in the previous paragraph.

Darwin's Uniformitarianism

A final minor point is just a caution that needs to be added to the generalization that Darwin made about "slow accumulation of small causes that could have world-shaping effects if given enough time" (page 6415, line 15-16). This is a form of uniformitarianism that Darwin derived from the work of his colleague Charles Lyell (1830). Evolution is about change, but that change does not necessarily have to be by the Darwinian mechanism of natural selection, which involves very long time scales. There are kinds of evolution in which rapid change occurs though extreme events that punctuate otherwise uniform rates. One should not exclude *a priori* the possibility that hydrological science, pursued in the mode of the historical sciences that Darwin's work exemplifies, might discover such modes of change in drainage basins. Indeed, one the major concerns about the nature of future hydrological change on the planet is that various kinds of extreme events may play an especially prominent role.

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